

EXHIBIT G

Redacted Sur-Rebuttal Expert Report of Robert H. Topel

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UNITED STATES DISTRICT COURT FOR THE DISTRICT OF NEVADA

CUNG LE, et al.,

Plaintiffs,

v.

ZUFFA, LLC d/b/a ULTIMATE
FIGHTING CHAMPIONSHIP and UFC,

Defendants.

Case No. 2:15-cv-01045-RFB-PAL

Sur-Rebuttal Expert Report of Professor Robert H. Topel

February 12, 2018

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I. INTRODUCTION

1. I am Robert H. Topel, the Isidore Brown and Gladys J. Brown Distinguished Service Professor of Economics at The University of Chicago Booth School of Business. I have been retained by counsel for Zuffa, LLC d/b/a Ultimate Fighting and UFC (collectively, “Zuffa”) to serve as an expert in economics in the above-captioned case. My qualifications were described in my initial report in this matter,¹ and I have attached an updated curriculum vitae as Appendix A to this report.

2. My initial report responded to the August 31, 2017 report filed by Dr. Hal J. Singer on behalf of Plaintiffs in this matter.² Recently, Dr. Singer filed a rebuttal report that offers new analyses in support of his opinions.³ I have been asked by counsel for Zuffa to respond to the new analyses offered by Dr. Singer in his rebuttal report.

3. My work is ongoing, and I will supplement it if I become aware of new information that affects my conclusions. The materials that I relied on in forming my opinions are listed in Appendix B and are cited throughout my report. The complete details of the calculations that I describe in this report are contained in the computer programs that accompany the report. In conjunction with the databases listed in Appendix B, these computer programs can be used to replicate the calculations referenced in my report.

II. DR. SINGER’S REVISED IMPACT REGRESSION**A. DR. SINGER’S NEW JUSTIFICATIONS FOR USING EVENT REVENUE INSTEAD OF THE MARGINAL REVENUE PRODUCT OF ATHLETES ARE INCORRECT AS A MATTER OF ECONOMICS**

4. According to Dr. Singer, his “central econometric analysis” is his impact regression, which he argues establishes that “Zuffa suppressed the wage share of Fighters as its dominance

¹ Expert Report of Professor Robert H. Topel, *Cung Le, et al., v. Zuffa, LLC d/b/a Ultimate Fighting Championship and UFC*, Case No. 2:15-cv-01045-RFB-PAL (D.Nev.) (October 27, 2017) [hereinafter TOPEL REPORT] at § I.A. and Appendix B.

² Expert Report of Hal J. Singer, Ph.D., *Cung Le, et al., v. Zuffa, LLC d/b/a Ultimate Fighting Championship and UFC*, Case No. 2:15-cv-01045-RFB-PAL (D.Nev.) (August 31, 2017) [hereinafter SINGER REPORT].

³ Rebuttal Expert Report of Hal J. Singer, Ph.D., *Cung Le, et al., v. Zuffa, LLC d/b/a Ultimate Fighting Championship and UFC*, Case No. 2:15-cv-01045-RFB-PAL (D.Nev.) (January 12, 2018) [hereinafter SINGER REBUTTAL REPORT].

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of the market increased, just as economics would predict.”⁴ In my initial report, I explained why Dr. Singer’s impact regression is inherently incapable of assessing the effects of Zuffa’s alleged monopsony power.⁵ In his rebuttal report, Dr. Singer responds to my criticisms of his impact regression by advancing several new arguments. As an initial matter, Dr. Singer now articulates that it is important to measure athletes’ compensation relative to their marginal revenue product (“MRP”): “If one could observe a Fighter’s MRP directly, then estimating the deviation of that Fighter’s wage from his MRP would inform the degree to which Zuffa exercises monopsony power over Fighters: The bigger the gap between a Fighter’s MRP and her wage, the more monopsony power the firm is exercising. However, as economists recognize, MRP is not directly observable.”⁶ I largely agree with Dr. Singer on this point. If one could reliably measure the MRP of Zuffa’s athletes, a comparison of those athletes’ compensation to their MRP could be informative in assessing whether, or to what extent, the Challenged Conduct anticompetitively reduced MMA athletes’ compensation. But Dr. Singer does not propose a methodology for reliably measuring the MRP of athletes, and, absent this information, Dr. Singer’s analyses remain completely uninformative regarding Plaintiffs’ claims.

1. Event Revenue Is Not an Appropriate Proxy for Athletes’ MRP

5. In his rebuttal report, Dr. Singer argues that event revenues can serve as a substitute for athletes’ MRP in his impact regression: “[T]he amount of revenue generated at a Live MMA Event is a reasonable proxy for the MRP of the Fighters competing in that event. Thus, the next best type of evidence of Zuffa’s monopsony power is to examine a Fighter’s compensation as a share of Event Revenue (the wage share).”⁷ Dr. Singer is incorrect. Event revenue is not a proxy for athletes’ MRP, and the ratio of an athlete’s compensation to event revenue is not an indicator or measure of the exercise of monopsony power. As a result, Dr. Singer’s impact regression

⁴ SINGER REBUTTAL REPORT at ¶ 3.

⁵ TOPEL REPORT at § VI.

⁶ SINGER REBUTTAL REPORT at ¶ 8.

⁷ SINGER REBUTTAL REPORT at ¶ 8.

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model cannot measure the degree to which Zuffa might have suppressed its athletes' compensation below competitive levels. It is simply uninformative.

6. To see the problem with Dr. Singer's use of event revenues as a proxy for Zuffa's athletes' MRPs, consider Dr. Singer's own definition of MRP: "the increase to a firm's revenues caused by the increase to the firm's output produced by the last worker employed, holding all else constant."⁸ Though this definition is awkwardly worded and incomplete, the key phrase is "holding all else constant." Applied to the current matter, the MRP of an athlete is the amount by which Zuffa's revenue—measured over some period such as the length of a contract or an event—increases when the athlete is added to the stock of other athletes under contract with Zuffa, holding constant all other things that determine Zuffa's productivity and revenue. Among the "other things" that must be held constant are Zuffa's promotional investments and efforts (both in its athletes and in its events), the number and quality of events, and the number and quality of all other athletes in Zuffa's roster.

7. Suppose we are interested in measuring the MRP of a particular athlete in a particular event. Conceptually, this can be measured in two steps. First, calculate the event's total revenues, which will reflect (1) the athlete's MRP, (2) the MRPs of other athletes who fought at the event, (3) the MRPs of all of Zuffa's investments and promotional activities, including the promotion of participating athletes, and (4) the contributions of other firms (*e.g.*, advertising by the venue or broadcaster). Second, calculate the hypothetical revenue if Zuffa had staged a second event that was identical to the first event in all respects, except that the event excluded the athlete whose MRP we want to measure, who is replaced by someone else.⁹ Revenues from the second, hypothetical event, will reflect other athletes' MRPs and the contributions of Zuffa and other firms to the event. The difference between actual event revenues and the event revenues of the second, hypothetical event is the athlete's marginal contribution to the event's revenues. Or, in other words, the athlete's MRP.

⁸ SINGER REBUTTAL REPORT at ¶ 8.

⁹ One could assume that the athlete whose MRP we wanted to measure was replaced in the event by one of a group of generic MMA athletes who are in elastic supply, so that this MRP calculation measures the value of the athlete over a replacement.

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8. In estimating his impact regression, Dr. Singer entirely ignores the second step in calculating an athlete's MRP: he confuses the total revenue from an event with the *change* in revenues that is solely attributable to the presence of an athlete. As a result, in constructing the dependent variable for his impact regression—athlete compensation divided by event revenue—the denominator, event revenue, does not reflect, measure, or proxy an athlete's MRP. In particular, the denominator of Dr. Singer's dependent variable also reflects Zuffa's own investments and efforts in increasing the value of the events and athletes it promotes. Event revenues increase when Zuffa's investments are successful. These investments raise the demand for Zuffa-promoted events and also raise the share of highly rated athletes that contract with Zuffa, which Dr. Singer wrongly describes as "foreclosure."

2. Controlling for Event Revenues Does Not Bias My Regression Results

9. In his rebuttal report, Dr. Singer argues that using his impact regression to estimate the relationship between "foreclosure share" and Zuffa's athletes' compensation in dollars would be "highly misleading" if the regression model does not properly account for Zuffa's increasing event revenues.¹⁰ Dr. Singer argues that when I estimated versions of his impact regression that use compensation levels as the dependent variable but *do not* control for Zuffa's event revenues, I am not properly accounting for the effect of athletes' increasing (so he speculates) MRPs into the regression analysis.¹¹ But, manufacturing an intractable dilemma, Dr. Singer argues that when I *do* control for Zuffa's event revenues in versions of his impact regression that use compensation levels as the dependent variable, I am "assum[ing] away the very hypothesis that [the] impact regressions are designed to test," since the regressions are "holding Event Revenue constant."¹² And in case this argument does not stick, Dr. Singer throws in a kitchen-sink claim that when I *do* control for Zuffa's event revenues, my regressions suffer from "a technical econometric problem called endogeneity."¹³ These arguments make no sense.

¹⁰ SINGER REBUTTAL REPORT at ¶ 109.

¹¹ SINGER REBUTTAL REPORT at ¶ 109.

¹² SINGER REBUTTAL REPORT at ¶ 117.

¹³ SINGER REBUTTAL REPORT at ¶ 116.

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10. Consider Dr. Singer’s first argument, in which he complains that, when I estimated versions of his impact regression using compensation as the dependent variable that did not control for event revenue, I “assume[d] nonsensically that Zuffa’s Event Revenue is unrelated to Fighter MRP, Fighter compensation, or both.”¹⁴ This statement is wrong—I made no such assumption, either explicitly or implicitly. Dr. Singer’s economic argument is that Zuffa’s alleged monopsony power, as measured by his contrived “foreclosure share”, reduced the pay of Zuffa athletes. The regressions I ran using compensation as the dependent variable—both with and without controlling for event revenue—show no relationship between Dr. Singer’s “foreclosure share” and athletes’ pay, in direct contradiction of Plaintiffs’ central claim. As a matter of econometrics, omitting event revenue from the regression will bias the impact of “foreclosure” only if (1) event revenue and Dr. Singer’s “foreclosure share” are correlated within years, and (2) changes in event revenue cause changes in athlete compensation.

11. Dr. Singer’s second argument criticizes specifications where I do control for event revenue in corrected versions of his impact regression using compensation as the dependent variable. He argues that controlling for event revenues means the regressions measure the effect of Zuffa’s foreclosure shares on athletes’ compensation “holding Event Revenue constant.”¹⁵ This is a virtue, not an indictment. The regression measures the relationship between Dr. Singer’s “foreclosure shares” and athletes’ compensation by comparing athletes’ compensation at events when Dr. Singer’s “foreclosure share” is high to athletes’ compensation levels at similar (in terms of revenue) events that occurred when Dr. Singer’s “foreclosure share” is low. In other words, the regression answers the question: If Zuffa earned \$10 million in revenue at an event they promoted when their “foreclosure share” was low, how does athlete compensation at that event compare to athlete compensation at a \$10-million event they promoted when their “foreclosure share” was high? By conditioning on (or “controlling for”) event revenues when

¹⁴ SINGER REBUTTAL REPORT at ¶ 111.

¹⁵ SINGER REBUTTAL REPORT at ¶ 117.

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making this comparison (*i.e.*, looking at athlete compensation for \$10-million events), the regression performs the economically and conceptually correct experiment.¹⁶

12. Dr. Singer's final argument is that event revenues are "endogenous," which somehow casts doubt on the impact regressions that control for event revenues.¹⁷ Although he does not explain the nature of this alleged endogeneity, as an econometric matter this would be a concern only if (1) there are factors that affect athletes' compensation that are excluded from Dr. Singer's impact regression, and (2) those omitted factors are correlated with both event revenues and Dr. Singer's "foreclosure share." Dr. Singer does nothing to explain what these sources of endogeneity might be. And, in any case, the remedy for this newly articulated endogeneity concern is not to divide compensation by event revenue, which is what Dr. Singer does. This ratio will mechanically decline if athletes' compensation rises more slowly than event revenue, and there is nothing in economic analysis indicating that a decline in this ratio is evidence of monopsony power. This is the core flaw in Dr. Singer's impact regression.

B. THE ECONOMIC LITERATURE NEWLY CITED BY DR. SINGER DOES NOT SUPPORT THE USE OF WAGE SHARE TO ASSESS THE EXISTENCE OF MONOPSONY POWER

13. In his rebuttal report, Dr. Singer argues that studies in the economic literature support his analytical approach because economists have used athlete compensation expressed as a fraction of revenue to analyze the impact of known changes in monopsony power, and because economists make reference to "wage share" in other contexts.¹⁸

14. However, the studies cited by Dr. Singer do not support his analytical approach. First, many of the studies cited by Dr. Singer use an analytical approach that is different from the one Dr. Singer uses. Second, the studies cited by Dr. Singer do not support his approach to regression analysis. Third, references to wage share in other contexts do not support the use of wage share to diagnose the existence of monopsony power. Fourth, in contrast to Dr. Singer's approach,

¹⁶ SINGER REBUTTAL REPORT at ¶ 109.

¹⁷ SINGER REBUTTAL REPORT at ¶ 116.

¹⁸ SINGER REBUTTAL REPORT at ¶¶ 88-104.

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many of the studies cited by Dr. Singer do not purport to diagnose the existence of monopsony power.

1. The Studies Cited by Dr. Singer Use an Analytical Approach that is Different from the One Dr. Singer Uses

15. Dr. Singer's analysis hinges on the assumption that in a competitive marketplace, athlete compensation and event revenue vary in proportion to one another. Dr. Singer claims that this assumption stems from his assertion that event revenue is driven primarily by athletes' efforts, so that event revenue is a useful proxy for the marginal revenue product of labor.¹⁹ Under this assumption and further assuming that all workers have the same MRP, in a competitive marketplace compensation and revenue will be jointly determined by the MRP and vary in proportion to one another.

16. But many of the articles that Dr. Singer cites do not make these assumptions. Instead, these articles estimate the MRP, based on its formal definition, and then compare the MRP to compensation. See for example Berri et al. (2015), Cassing and Douglas (1980), Raimondo (1983), Scully (1974), Scully (1989), Scully (2004), and Sommers and Quinton (1982); see also Bradbury (2013) and Kahn (2000).²⁰ That is, there is an important difference in the approach

¹⁹ SINGER REBUTTAL REPORT at ¶¶ 8, 72.

²⁰ Other articles that Dr. Singer cites take yet a different approach. Using wages as the dependent variable, Krautmann (1999) estimates the compensation structure of free agents and uses that structure to benchmark what non-free agents would be paid in a setting with reduced monopsonistic constraints. Hill and Spellman (1983) compare the compensation structure of free agents to non-free agents, using log of compensation as the dependent variable. David Berri, Michael Leeds, and Peter von Allmen, "Salary Determination in the Presence of Fixed Revenues", *International Journal of Sport Finance*, vol. 10, no. 1 (2015); James Cassing and Richard W. Douglas, "Implications of the Auction Mechanism in Baseball's Free Agent Draft", *Southern Economic Journal*, vol. 47, no. 1 (1980); Henry J. Raimondo, "Free Agent's Impact on the Labor Market for Baseball Players", *Journal of Labor Research*, vol. 4, no. 2 (1983); Gerald W. Scully, "Pay and Performance in Major League Baseball", *The American Economic Review* vol. 64, no. 6 (1974); Gerald W. Scully, *The Business of Major League Baseball*, (University of Chicago Press 1989) at pp. 151-170; Gerald W. Scully, "Player Salary Share and the Distribution of Player Earnings", *Managerial and Decision Economics*, vol. 25, no. 2 (2004); Paul M. Sommers and Noel Quinton, "Pay and Performance in Major League Baseball: The Case of the First Family of Free Agents", *The Journal of Human Resources*, vol. 17, no. 3 (1982); John Charles Bradbury, "What is Right with Scully Estimates of a Player's Marginal Revenue Product", *Journal of Sports Economics*, vol. 14, no. 1 (2013); Lawrence Kahn, "The Sports Business as a Labor Market Laboratory", *Journal of Economic Perspectives*, vol. 14, no. 3 (2000). Anthony Krautmann, "What's Wrong with Scully-Estimates of a Player's Marginal Revenue Product", *Economic Inquiry*, vol. 37, no. 2 (1999); James R. Hill and William Spellman, "Professional Baseball: The Reserve Clause and Salary Structure", *Industrial Relations*, vol. 22, no. 1 (1983).

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these articles take relative to Dr. Singer's approach: Dr. Singer's approach requires the assumption that athletes are the only material drivers of revenue, which is simply untrue. In contrast, the approach used in the articles cited by Dr. Singer does not require this assumption; instead, these articles estimate MRP after accounting for other determinants of revenue.

17. Indeed, several of these articles highlight the importance of accounting for other determinants of revenue in order to allow for an accurate assessment of labor's impact on revenue and thus an accurate comparison between an athlete's MRP and his or her compensation. Scully (1974) notes that in baseball, marginal revenue product is driven not only by athlete performance but also by factors including managerial performance and entrepreneurial player drafting and trading abilities.²¹ Scully (2004) notes the impact on marginal revenue product by factors including managers, coaches, scouts, advertising, and promotions. In the same article, Scully notes that the growth in NFL non-designated revenues, of which players are not guaranteed a share, contributed to the reduction in athletes' share of total (designated and non-designated) revenue.²² Krautmann (1999) highlights the impact of factors such as managerial staff on team performance and thus revenue.^{23,24} These are only three examples—the point is far more general and important.

2. The Studies Cited by Dr. Singer Do Not Support His Approach to Regression Analysis

18. None of the articles that Dr. Singer cites use regression analysis with labor share of revenue as the dependent variable to measure the anticompetitive effect, impact, or damages resulting from monopsonistic conduct. Dr. Singer's reliance on Autor et al. (AER, 2017) is particularly misleading.²⁵ The authors find that revenue and employment have become

²¹ Scully (1974) at p. 921.

²² Scully (2004) at pp. 79, 84.

²³ Krautmann (1999) at p. 370.

²⁴ Other revenue drivers often controlled for include a measure of wins, population, stadium capacity, stadium age, the team's racial composition, and an expansion team indicator.

²⁵ David Autor, David Dorn, Lawrence Katz, Christina Patterson, and John Van Reenen, "Concentrating on the Fall of the Labor Share", *American Economic Review: Papers & Proceedings*, vol. 107, no. 5 (2017); see also David Autor, David Dorn, Lawrence F. Katz, Christina Patterson, and John Van Reenen, "The Fall of the Labor Share and the Rise of Superstar Firms," Working Paper (May 1, 2017).

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increasingly concentrated among large firms over time in the U.S., and that the labor share of revenue has fallen more in industries where concentration has increased most. But the authors do not infer the existence of anticompetitive activity from these relationships. Rather, they consider both procompetitive and anticompetitive hypotheses, and devise a test to distinguish between the two. They explain that the anticompetitive explanations imply that the firms with low labor share and increasing concentration would have lower productivity than other firms; in contrast, the procompetitive explanations imply that the firms with low labor share and increasing concentration would have higher productivity than other firms. With this test, they find that the latter explanation is consistent with the evidence: firms that gained market share in the product market and that have low labor shares have high total factor productivity, high rates of output per worker, high rates of patents per worker, and high value-added per worker. They are “superstar” firms, according to the authors. Autor et al. conclude that the decline in labor’s share of GDP has been driven by procompetitive (rather than anticompetitive) activity—the growth of superstar firms. Note that this conclusion is nearly identical to my conclusions regarding Zuffa’s conduct, explained in detail in my initial report. Because Zuffa was more productive than other MMA promoters, its event revenues and share of top athletes increased. Compensation of athletes also rose, but compensation as a share of revenues declined. This is not evidence of monopsony power.

3. References to Wage Share in Other Contexts Do Not Support the Use of Wage Share to Diagnose the Existence of Monopsony Power

19. In addition to the forgoing errors, Dr. Singer cites articles which refer to the use of wage share in contexts unrelated to the diagnosis of monopsony power. These articles are unrelated to Dr. Singer’s present endeavor and provide no support for Dr. Singer’s opinions.

20. For example, Dr. Singer notes that a 2009 study I coauthored with a colleague at The University of Chicago refers to wage share.²⁶ But this study does not discuss monopsony power. The purpose of this study was to determine whether, as a financial matter, NFL teams could afford to pay players the amount set forth in the previously agreed-upon 2006 extension of the

²⁶ Kevin Murphy and Robert Topel, “The Economics of NFL Team Ownership,” Chicago Partners, (2009).

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Collective Bargaining Agreement (“CBA”). The study does not analyze how much NFL players should be paid, or would be paid in a competitive marketplace, it only analyzes whether NFL teams could afford to pay players the amounts that had been agreed to in the CBA—*i.e.*, whether they were losing money under the current agreement, which they had alleged. Because NFL owners had complained about the ratio of player salaries to team revenues, the study addresses this question by presenting statistics on players’ share of post-CBA revenue. As such, the study provides no support for Dr. Singer’s analytical approach.²⁷

21. Similarly, Dr. Singer cites an example in a textbook (Ruffin, et al. (1993)) which documents how much workers have been paid relative to earnings in the private business sector between 1948 and 1990.²⁸ The example does not discuss monopsony power; instead, it discusses how in a competitive marketplace, labor is paid the marginal revenue product of labor, and the price of capital reflects the marginal revenue product of capital. Notably, the example highlights the fact that both labor and capital contribute to business earnings; this is in contrast to Dr. Singer’s approach, which hinges on the assumption that only athletes have a material impact on revenue.

4. In Contrast to Dr. Singer’s Approach, Many of the Studies Cited by Dr. Singer Do Not Purport to Diagnose the Existence of Monopsony Power

22. Dr. Singer attempts to infer the existence of monopsony power using his impact regression. In contrast, many of the studies cited by Dr. Singer in his rebuttal report do something different: they analyze the impact of an observed change in known monopsony power on economic outcomes, rather than analyzing changes in some outcome (*e.g.*, compensation) to infer the existence of changes in monopsony power. The analyses undertaken in the studies cited by Dr. Singer are much better defined than Dr. Singer’s own analysis. This is because a known change in monopsony power—such as eliminating the reserve clause in baseball—has clear implications for compensation: all else equal, a reduction in monopsony power will increase wages. Many of the articles and books Dr. Singer cites focus on the targeted question of the impact of a known change in monopsony power, including Hill and Spellman (1983), Kahn

²⁷ Murphy and Topel (2009).

²⁸ Roy Ruffin and Paul Gregory, *Principles of Microeconomics* (Harper Collins, 5th Edition, 1993) at pp. 331-36.

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(2000), Krautmann (1999), Monks (2013), Noll (1988), Pindyck and Rubinfeld (2013), Raimondo (1983), Rittenberg and Tregarthen (2009), Scully (1974), Scully (1978), Scully (2004), Sommers and Quinton (1982), and Twomey and Monks (2011).²⁹ (Furthermore, it is worth noting that several of the articles Dr. Singer cites are not focused on the subject of monopsony power, or at most mention it as a minor aside, including De Loecker and Eeckhout (2017), Elsby et al. (2013), Fort and Noll (1984), Horowitz (1974), Noll (1974), and Vrooman (2012).³⁰)

23. In contrast, the building blocks of Dr. Singer's impact regression are changes in athlete compensation, event revenue, and "foreclosure share." The fundamental economic problem with Dr. Singer's approach is that the observed relationship among these metrics—compensation as a fraction of event revenue declines as "foreclosure" rises—would occur even if Zuffa's conduct was entirely procompetitive, and even if Zuffa exercised no substantive market power. For example, as I explained in my original report, a negative correlation between wage share (in this case, athletes' compensation divided by event revenue) and the fraction of top athletes under

²⁹ James Monks, "Revenue Shares and Monopsonistic Behavior in Intercollegiate Athletics", *Cornell Higher Education Research Institute Working Paper 155* (September 2013); Roger G. Noll, *The Economics of Sports Leagues*, in *Law of Professional and Amateur Sports*, (Gary A. Uberstine ed., Clark Boardman 1988); Robert Pindyck and Daniel Rubinfeld, *Microeconomics* (Pearson 8th Edition. 2013) at p. 549; Libby Rittenberg and Timothy Tregarthen, *Principles of Economics* (Flat World Knowledge 2009), at p. 356; Gerald W. Scully, "Binding Salary Arbitration in Major League Baseball", *American Behavioral Scientist*, vol. 21, no. 3 (1978); John Twomey and James Monks, "Monopsony and Salary Suppression: The Case of Major League Soccer in the United States", *The American Economist*, vol. 56 no. 1 (2011). See also Bradbury (2013); Roger Blair, *Sports Economics* (Cambridge University Press 2012) at p. 352.

³⁰ Jan De Loecker and Jan Eeckhout, "The Rise of Market Power and the Macroeconomic Implications", Working Paper (August 24, 2017); Michael W.L. Elsby, Bart Hobijn, and Aysegul Sahin, "The Decline of the U.S. Labor Share", *Brookings Papers on Economic Activity* (2013); Rodney D. Fort and Roger G. Noll, "Pay and Performance in Baseball: Modeling Regulars, Reserves and Expansion", *California Institute of Technology Social Science Working Paper 527* (May 1984); Ira Horowitz, *Sports Broadcasting*, in *Government and the Sports Business* (Roger G. Noll ed., Brookings Institution, 1974); Roger G. Noll, *Attendance and Price Setting*, in *Government and the Sports Business* (Roger G. Noll ed., Brookings Institution 1974); John Vrooman, *The Economic Structure of the NFL*, in *The Economics of the National Football League: The State of the Art* (Kevin G. Quinn ed., Springer 2012). In Dobbelaere et al. (2013), one objective of the analysis is to identify the existence of monopsony power, but the authors recognize that such power can arise from transaction costs or worker preferences for particular job characteristics, and the authors do not attempt to identify anticompetitive activity. Also, the approach used in Dobbelaere et al. (2013) does not involve a regression with labor share of revenue as the dependent variable. (Sabien Dobbelaere and Jacques Mairesse, "Panel Data Estimates of the Production Function and Product and Labor Market Imperfections", *Journal of Applied Econometrics*, vol. 28, no. 1 (2013)).

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contract with Zuffa would occur if Zuffa successfully promoted MMA, its brand and events, increasing event revenues and attracting more athletes. Then compensation as a fraction of event revenue will decline because revenues rise faster than compensation. In fact, Zuffa's expansion was accompanied by a very substantial *increase* in the average compensation of Zuffa athletes—the opposite of what Plaintiffs' monopsony theory predicts. Dr. Singer's declining ratio of compensation to event revenue hides the fact that compensation was rising. As such, to establish that monopsony power explains the negative relationship between wage share and “foreclosure share,” Dr. Singer must consider procompetitive reasons for the identical outcome and demonstrate that the evidence clearly rejects those procompetitive reasons. He has completely failed to do so.^{31,32}

C. DR. SINGER'S NEW ECONOMETRIC TEST REGARDING THE USE OF STRIKEFORCE AS A BENCHMARK FOR ZUFFA IS INCORRECT

24. Dr. Singer argues in his rebuttal report that his impact regression incorporates two sources of variation in athletes' compensation: (1) variation over time in the fraction of event revenue that Zuffa paid to its athletes as its “foreclosure share” changed, and (2) variation in the fraction of event revenue that Strikeforce athletes received before and after Strikeforce was

³¹ An example of the kind of balanced assessment which is required is demonstrated in the aforementioned studies by Autor et al. (AER, 2017) and Autor et al. (working paper, 2017). In contrast, another article that Dr. Singer cites (Vrooman (2009)) illustrates the number of competing theories that can explain a given phenomenon and serves as a cautionary tale about the importance of testing each theory before concluding that one of them is correct. Vrooman observes an increase in the ratio of compensation to revenue in various sports leagues, states a theory that he believes explains these patterns – which is that sports team owners are not profit-maximizers – and concludes that the owners' reduced focus on profit has allowed for higher wage shares: “Clear evidence that the players' share has approached two-thirds of revenue in all four majors sports leagues leads to the surprising conclusion that monopsony power in all leagues has virtually vanished. There are several explanations, ranging from new-found bargaining power of free-agent players to external competition of rival leagues. The most plausible argument is that the league-cartel solidarity has been compromised by internal competition from the profit-max (Nash) behavior of individual owners or by win-max objectives of sportsman owners. Over time a pure-sportsman league is the steady state, in which profit max owners can either change their objectives or be driven from the league...” (John Vrooman, “Theory of the Perfect Game: Competitive Balance in Monopoly Sports Leagues”, *Review of Industrial Organization*, vol. 34, no. 1, (2009) at p. 27)

³² Scully (1989) considers whether baseball owners colluded to reduce compensation for the 1985, 1986, and 1987 crops of free agents. To test this, Scully regresses the log of player salary on measures of player performance, experience, contributions, and a dummy variable indicating whether the player was a free agent and finds that free agency had a negative impact on player salaries. Scully also estimates players' MRP, compares the MRP to salaries, and finds that the ratio of salary to MRP was lower for free agents than for non-free agents. Scully also briefly notes that a reduction in player salary as a share of revenue after 1981 could be interpreted as an indicator of owner collusion.

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acquired by Zuffa.³³ Dr. Singer attempts to salvage the use of this second source of variation, his putative Strikeforce “benchmark”, by offering a new econometric analysis. I will address the errors in that analysis shortly. But even if Dr. Singer’s new analysis were correct (and it is not), his analysis is only as good as the data used to estimate the alleged impact of Zuffa’s “foreclosure.” Dr. Singer constructs his “foreclosure share” as the share of MMA athletes at any time who are under contract with Zuffa, where, according to his calculations, the contract is at least 30 months in length and the contract contains a champion’s clause.³⁴ By the end of the Class Period, Dr. Singer claims that this “foreclosure share” exceeded 70 or 80 or 90 percent.³⁵ Remarkably, in defining the “foreclosure share” for pre-acquisition Strikeforce bouts, he assigned each such bout a “foreclosure share” of zero, which is completely arbitrary.³⁶ This implies that his so-called Strikeforce “benchmark” assumes that Zuffa had no athletes under contract, which is absurd. Dr. Singer then uses these clearly erroneous data in estimating his impact regression, which yields an inflated estimate of “impact.” Without these erroneous Strikeforce data, his model yields no evidence of anticompetitive impact.³⁷ Inclusion of these erroneous data means that Dr. Singer’s estimates are completely unreliable and misleading, no matter what econometric machinations he may proffer.

25. Dr. Singer responded to my criticism of his use of pre-acquisition Strikeforce bouts in his rebuttal report by estimating a new impact model that he argues is statistically valid. Dr. Singer does not dispute the appropriateness of the Chow test I used to determine whether the pre-

³³ SINGER REBUTTAL REPORT at ¶¶ 73-75.

³⁴ SINGER REPORT at ¶¶ 171-172, 182.

³⁵ SINGER REPORT at Figure 3.

³⁶ SINGER BACKUP.

³⁷ The most straightforward method of correcting Dr. Singer’s error in the context of his impact regression is to simply exclude the pre-acquisition Strikeforce bouts when estimating the regression. After doing so, two of Dr. Singer’s three regression models imply that there is no statistically significant relationship between compensation as a share of revenue and foreclosure. (TOPEL REPORT at § VII.B.2.) Alternatively, Dr. Singer could have retained the pre-acquisition Strikeforce bouts but included in his regression model an additional indicator variable that distinguishes pre-acquisition Strikeforce bouts from post-acquisition Strikeforce bouts. Inclusion of this indicator variable prevents the “zeroes” in the “foreclosure share” for pre-acquisition Strikeforce bouts from contaminating Dr. Singer’s estimate of the “foreclosure” effect. In my backup materials, I add this variable to Dr. Singer’s impact regression model. When I do so, the estimated coefficients on Dr. Singer’s measures of foreclosure (Tracked, Ranked, and Headliner) become insignificant.

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acquisition Strikeforce bouts should be used to estimate his impact regression. Instead, he implements an alternative, non-standard version of the Chow test that appears to be engineered to produce a negative, statistically significant correlation between Dr. Singer's "foreclosure share" and athlete compensation.³⁸ To do this, Dr. Singer first estimates the same flexible regression model that formed the basis of the Chow test in my initial report (*i.e.*, he includes all 63 Strikeforce interaction variables that I used in my initial report).³⁹ Then, without justification or explanation, he applies the Chow test to only 19 of the 63 interaction variables instead of all 63. When Dr. Singer tests the joint statistical significance of these 19 interactions, he finds that they are jointly statistically insignificant. Concluding that it would be inappropriate to include these 19 interactions, he re-estimates his impact model excluding them (*i.e.*, including only 44 of the 63) and he finds a negative, statistically significant relationship between "foreclosure share" and athletes' compensation.⁴⁰

26. This is statistical gerrymandering. Dr. Singer provides no justification or explanation in either his rebuttal report or backup as to why, of the more than nine quintillion combinations of interactions he could have selected for this test, he settled on the one combination of 19 interactions that he did.⁴¹ While Dr. Singer characterizes these 19 interactions as "statistically identical for Strikeforce pre-acquisition bouts and Zuffa bouts," suggesting to the reader that these 19 coefficients were individually statistically insignificant, this is incorrect.⁴² Several of the 19 interactions Dr. Singer selects are individually highly statistically significant, and many of the 44 interactions Dr. Singer does not select are individually highly statistically insignificant.

27. To demonstrate the problems with the approach that Dr. Singer apparently adopted, it is straightforward to data-mine alternative subsets of the 63 interaction variables so that (1) the interaction variables I select are jointly statistically insignificant, and (2) there is no statistical

³⁸ SINGER REBUTTAL REPORT at § II.B.3.

³⁹ TOPEL REPORT at ¶¶ 156

⁴⁰ SINGER REBUTTAL REPORT at ¶ 85.

⁴¹ Since there are 63 interaction variables, Dr. Singer could have applied his approach to $2^{63}-2$ subsets of the 63 explanatory variables.

⁴² SINGER REBUTTAL REPORT at ¶ 85.

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evidence of a correlation between “foreclosure share” and athletes’ compensation as a share of event revenue.⁴³ As but one example, I selected the 15 interaction variables that are least statistically significant within the fully interacted model.⁴⁴ These 15 interaction variables are jointly statistically insignificant. Re-estimating Dr. Singer’s impact regression excluding these 15 interactions—so now it includes 48 of the 63 interactions—shows that there is no statistically significant negative relationship between “foreclosure share” in Dr. Singer’s Tracked or Headliner market and athletes’ compensation as a share of event revenue.⁴⁵

28. In rebuttal, Dr. Singer also argues that the results of the Chow test cannot be used to “discard” data.⁴⁶ While Dr. Singer’s language is pejorative, the Chow test is, in fact, expressly designed to identify situations in which certain data should not be used to estimate an econometric model. In the event that data should not be included, an economist can either (1) “discard” the inappropriate data or (2) estimate a fully interacted model. The results of these two options are essentially econometrically equivalent. More importantly, as I discussed above, the pre-acquisition Strikeforce data in question were clearly miscoded by Dr. Singer, and should not be used to estimate his impact regression in any case.

D. DR. SINGER’S NEW REGRESSION USING STRATIFIED SHARES IS INCORRECTLY SPECIFIED

29. In this section, I discuss Dr. Singer’s new analyses that attempt to account for differences in MMA athletes’ ability when calculating “foreclosure shares.” In his first report, Dr. Singer proposes accounting for these differences in two ways: (1) by weighting each athlete “by their associated promoters’ PPV and gate revenues per Fighter,” and (2) weighting by the inverse of

⁴³ I do not believe this approach is appropriate. I perform these regressions only to demonstrate that it is not difficult to modify Dr. Singer’s approach and generate results that are favorable to Zuffa.

⁴⁴ After estimating the model with 63 interactions, I calculate the *p*-value for each of the 63 interactions. I then identify the 15 interaction variables whose *p*-values are the largest (*i.e.*, those interactions that are least likely to be different from zero).

⁴⁵ When I use the “foreclosure share” from Dr. Singer’s Ranked market, the estimated coefficient on “foreclosure” is still statistically significant after I remove the 15 interactions from the regression. However, the estimated coefficient has increased from -0.043 to -0.019.

⁴⁶ SINGER REBUTTAL REPORT at ¶ 80.

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an athletes rank according to FightMatrix.⁴⁷ He argues that using these weights to calculate “foreclosure shares” is appropriate because “[u]sing unweighted Fighter counts makes little economic sense, given that Fighters (even within the defined Relevant Input Market and Submarket) are not homogenous, and instead vary in their ability to attract viewers—and hence their value to an MMA promoter.”⁴⁸ While I do not disagree with Dr. Singer that it may be appropriate to account for differences in athletes’ ability, I explained in my initial report that the approaches proposed by Dr. Singer are flawed and misleading.⁴⁹ To remedy the shortcomings in Dr. Singer’s analyses, I calculated unweighted “foreclosure shares” separately for five different groups (or, strata) of MMA athletes based on their rankings, and then replaced the revenue-weighted measure of “foreclosure” in Dr. Singer’s impact regression model with the unweighted shares from the five ranking strata.⁵⁰

30. In his rebuttal report, Dr. Singer claims my use of stratified “foreclosure shares” suffers from “methodological errors,” although he does not describe what those errors might be.⁵¹ Dr. Singer also estimates five new specifications of his impact regression model using stratified “foreclosure shares.”⁵² In each specification, Dr. Singer includes the measure of “foreclosure” for only one stratum of athlete, even though the regression is estimated using compensation data for all athletes. So, for example, in one of Dr. Singer’s new impact regressions the share of headliners under contract with Zuffa is allowed to affect the share of event revenue paid to lower-ranked athletes, but the share of lower-ranked athletes signed with Zuffa cannot affect their own compensation share. While he does not say so in his rebuttal report, in estimating these five new impact models, Dr. Singer uses *revenue-weighted* stratified “foreclosure shares,” rather

⁴⁷ SINGER REPORT at ¶ 128.

⁴⁸ SINGER REPORT at ¶ 128.

⁴⁹ TOPEL REPORT at §§ XIV.A – XIV.E.

⁵⁰ TOPEL REPORT at ¶¶ 165-166. I calculated Zuffa’s unweighted “foreclosure share” within five categories: headliners ranked between 1 and 15, and MMA athletes ranked between 16 and 30, 31 and 50, 51 and 100, and lower than 100.

⁵¹ SINGER REBUTTAL REPORT at ¶ 121.

⁵² SINGER REBUTTAL REPORT at § II.D.3 and Appendix Table A2.

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than the *unweighted* stratified foreclosure shares I used in my report.⁵³ Apart from his use of revenue-weighted shares, which are inappropriate for the reasons I described in my initial report, Dr. Singer's new impact regressions are flawed for two additional reasons.

31. First, Dr. Singer continues to incorrectly use pre-acquisition Strikeforce bouts in estimating these new impact regressions. Second, these regressions also no longer test whether athletes' compensation is affected differently if the share of higher- or lower-ranked athletes under contract with Zuffa changes. But this was the purpose of the regression I estimated in my initial report. I included "foreclosure" measures for each of the five strata in the same regression so that I could directly test whether Zuffa's contracts with athletes of differing abilities affected athlete compensation. For example, Dr. Singer argued in his initial report that Zuffa's share of "Headliners" (*i.e.*, athletes ranked in the top 15 in their respective divisions) gave it monopsony power over MMA athletes more broadly: "Given that Headliners are a critical input for staging successful Live MMA Events, Zuffa's foreclosure of Headliners impairs rivals and enables Zuffa to exercise monopsony power over all Fighters in the Relevant Input Market."⁵⁴ Dr. Singer's modified impact regressions cannot answer his own question, since they include a measure of foreclosure for only one strata of athletes at a time.

III. DR. SINGER'S NEW REGRESSION OF ATHLETES' COMPENSATION ON OTHER ATHLETES' COMPENSATION PROVIDES NO EVIDENCE OF A COMMON COMPENSATION STRUCTURE

32. In his initial report, Dr. Singer estimated the relationship between individual athlete's per-event compensation ("own compensation") and the average of other athletes' per-event compensation using Zuffa compensation data from between 2005 and 2016.⁵⁵ As I described in my initial report, limiting Dr. Singer's analysis to the Class Period—a change which is demanded by the question Dr. Singer is attempting to answer—reveals no evidence of any relationship between own and average others' per-event compensation.⁵⁶

⁵³ SINGER REBUTTAL BACKUP.

⁵⁴ SINGER REPORT at ¶ 167.

⁵⁵ SINGER REPORT at § IV.B.2.

⁵⁶ TOPEL REPORT at ¶¶ 268-271 and Exhibit 32.

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33. To rescue his earlier claims, in his rebuttal report, Dr. Singer for the first time proposes a limited test for a “structural break” between the pre-Class and Class Periods in estimating his “common” compensation structure regression.⁵⁷ This is an attempt to justify including pre-Class data in his regression. In conducting this structural break test of pre-Class and Class commonality, Dr. Singer improperly *assumes* that the pre-Class and Class Period structures of compensation are identical with the exception of a single parameter—the correlation of an athlete’s compensation with the average pay of other athletes. Because he finds that the coefficient on this one parameter is not statistically different in the pre-Class and Class Periods, he concludes that the structure of compensation was statistically the same in the two periods, and that his original pooled model was correct.

34. Dr. Singer’s conclusion is wrong. He assumes, without justification and without testing the assumption, that the coefficients on the other variables in his model—including the time trend, intercept and athlete fixed effects—are the same in the pre-Class and Class Periods. If the data indicate they are not the same, then his assumption of a common compensation structure in the two periods will be rejected, and his estimates of that structure will be biased.

35. An econometrics textbook referenced by Dr. Singer in his report states that this is precisely the context in which one would want to estimate a fully interacted model, allowing all the coefficients to vary, and perform a Chow test rather than the limited test Dr. Singer ran. Specifically, the authors discuss using a Chow test to test whether “a given model applies to two different data sets.”⁵⁸ The example used to illustrate this test is very similar to the context here: comparing a model estimated using data from two different time periods. The authors indicate that the rejection of the null “implies that two separate regressions must be estimated: the data cannot be pooled.”⁵⁹

36. I performed this test, comparing separate regression models estimated using data from the pre-Class and Class Periods. I tested whether the models are the same using the standard

⁵⁷ SINGER REBUTTAL REPORT at ¶ 162.

⁵⁸ Pindyck and Rubinfeld (1991) at p. 115.

⁵⁹ Pindyck and Rubinfeld (1991) at p. 116.

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statistical test—an *F*-test, sometimes called a Chow test in this context. The results of this test reject Dr. Singer’s assumption that the coefficients in the model are the same in the pre-Class and Class periods, which implies that pooling the pre-Class and Class period data is inappropriate.⁶⁰

IV. DR. SINGER’S NEW CAREER LENGTH CALCULATIONS ARE WRONG

37. In his rebuttal report, Dr. Singer responds that my 8.9-year career length calculation “is driven by Fighters when they are outside the Relevant Input Market and Submarket.”⁶¹ He supports his argument by comparing my calculation to an entirely new set of calculations of career length, which range from 0.38 years to 1.33 years (depending on whether a “career” is defined using Zuffa bouts only, or bouts in the Tracked, Ranked, or Headliner markets).⁶² While Dr. Singer implies that the difference between my career-length calculation of 8.9 years and his calculation of 1.33 years (for the Ranked market) is due to the inclusion of bouts outside of the Ranked market in my calculation, he is incorrect.

38. In calculating career lengths in his rebuttal report, Dr. Singer has adopted a different methodology and has chosen the wrong set of athletes, the wrong set of bouts to include in these athletes’ careers, or both. The effect of these errors is that Dr. Singer calculates career lengths in such a way that seems designed to make athletes’ careers as short as possible.

39. In Table 1 of his Rebuttal Report, Dr. Singer reports median career lengths for his Tracked, Ranked and Headliner markets (of 0.38, 1.33, and 1 years, respectively). In these calculations, Dr. Singer inexplicably calculates the median and mean career length across all athletes in the respective markets—*i.e.*, he includes athletes that never competed with Zuffa. The career length of athletes who never competed with Zuffa is completely irrelevant for understanding whether Zuffa contracts foreclosed rival access to athletes—clearly athletes that never had a Zuffa contract could not have been foreclosed by Zuffa. These non-Zuffa athletes

⁶⁰ Because some of the athlete fixed effects are identified with very few observations, I also perform a Chow test on a model where I do not include interactions of the pre-Class indicator with athlete fixed effects. In this model, I test the joint significance of the pre-Class indicator interacted with average others’ compensation, the time trend and the intercept, and strongly reject the null that the coefficients are equal in the pre-Class and Class periods.

⁶¹ SINGER REBUTTAL REPORT at ¶ 64.

⁶² SINGER REBUTTAL REPORT at ¶ 64 and Table 1.

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represented 60 percent of his sample in the Tracked Market, 85 percent of his sample in the Ranked Market, and 64 percent of his sample in the Headliner Market. Dr. Singer assigns these athletes equal weight to Zuffa athletes in his calculations. Since the non-Zuffa athletes tend to have shorter careers, this causes Dr. Singer to calculate much shorter career lengths. Dr. Singer provides no rationale for including these athletes, and provides no explanation as to why he has altered his methodology from his first report, which excluded these athletes. Because he uses a different (and incorrect) sample of athletes, Dr. Singer's career lengths for the Tracked, Ranked and Headliner markets are not comparable to my career length calculation.

40. Using Dr. Singer's exact methodology, but limiting the analysis to athletes with at least one Zuffa bout, I calculate a median career length of 5.9 years based on bouts in Dr. Singer's Ranked Market, compared to the 1.3 years calculated by Dr. Singer. In addition, using the same methodology, I calculate a median career length of 8.7 years based on all bouts in Sherdog. Thus, even accepting Dr. Singer's more narrow definition of a career as including only bouts in the Ranked Market, I find that the median Zuffa career is 2.7 times the 26-month median contract length I calculated in my first report, and twice the 35-month median contract length calculated by Dr. Singer, indicating that Zuffa contracts did not foreclose rivals.

41. Dr. Singer also calculates a median career length of 0.82 years for Zuffa athletes in Zuffa bouts. To state the obvious, an athlete's MMA career is not limited to the time the athlete spent at Zuffa. It is unclear why Dr. Singer includes this calculation in his career length analysis, but this calculation undermines Dr. Singer's conclusion regarding foreclosure. Dr. Singer considers an athlete foreclosed if the term of the contract (including various provisions like the Right to Match provision) is 30 months or greater.⁶³ Dr. Singer also calculates that the median contract length of Zuffa athletes is 35 months, meaning that nearly all Zuffa athletes are foreclosed.⁶⁴ The new calculations in his Rebuttal Report indicate the very opposite is the case—the short median career length at Zuffa (0.82 years) indicates that Zuffa typically does not keep athletes even as long as one-third of the median 35-month contract calculated by Dr. Singer and that Zuffa athletes are only contractually unavailable to competitors for short periods of time.

⁶³ SINGER REPORT at ¶ 171.

⁶⁴ SINGER REPORT at ¶ 89 and Table 1.

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V. DR. SINGER'S NEW REGRESSIONS SHOWING A CORRELATION BETWEEN ATHLETES' RANKINGS, COMPENSATION, AND EVENT REVENUE DO NOT SUPPORT HIS USE OF RANK WEIGHTS

42. In my initial report, I critiqued Dr. Singer's method of weighting his "foreclosure shares" using the inverse of an athletes' ranking.⁶⁵ In his Rebuttal Report, Dr. Singer includes two new regressions that he claims support his use of the inverse of athletes' rankings in weighting his "foreclosure share" calculations.⁶⁶ For reasons I discuss below, Dr. Singer's new analysis does not address my criticisms.

43. The first new analysis that Dr. Singer includes in his rebuttal report is a regression of event revenues on the inverse of the ranking of the top-ranked athlete at that event.⁶⁷ To estimate this regression, Dr. Singer first calculates the average event revenue across all Zuffa events in the Class Period separately according to the rank of the top-ranked athlete participating in the event. Because most Zuffa events during the Class Period included at least one highly ranked athlete, this aggregated data set has only 16 observations, with the ranking of the top-ranked athlete at each event ranging between one and 28.⁶⁸ So, for example, one observation in the dataset used to estimate his regression is the average event revenue across the 86 events where the top-ranked athlete had a number-one ranking. Another observation is the average event revenue for the two events where the top-ranked athlete had a number-28 ranking. Dr. Singer then regresses the average event revenues on the inverse ranking of the top-ranked athlete at the event, and finds a statistically significant positive relationship between the inverse ranking and average event revenue.⁶⁹

44. At most, Dr. Singer's new regression documents that, on average, events featuring higher-ranked athletes generate higher revenues. This is hardly surprising since events including, for example, championship bouts, are more heavily promoted by Zuffa, attract more media attention, and are held in larger venues. But this is not at all what Dr. Singer needs to

⁶⁵ TOPEL REPORT at § XIV.E.

⁶⁶ SINGER REBUTTAL REPORT at ¶ 130.

⁶⁷ SINGER REBUTTAL REPORT at ¶ 130.

⁶⁸ SINGER REBUTTAL BACKUP.

⁶⁹ SINGER REBUTTAL REPORT at ¶ 130.

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demonstrate to justify his use of the inverse of athletes' ranking in calculating his "foreclosure share." Rather, he needs to establish that the inverse of athletes' ranking is somehow related to Zuffa's ability to foreclose rivals.

45. There are other problems with Dr. Singer's new analysis as well. First, he assumes that the only determinant of event revenues is the rank of the top-ranked athlete (or that if there are other factors that contribute to event revenues, they are uncorrelated with the ranking of the top-ranked athlete). That is, Dr. Singer assumes that the revenue-producing effects of the lower-ranked athletes in an event are zero, and that the impact of Zuffa's promotional efforts and investments on event revenue are negligible. But if these assumptions were correct, then there should be substantially less variability in event revenue (conditional on the ranking of the top-ranked athlete) than he observes in the data. For example, there were 86 events in Dr. Singer's analysis that included an athlete ranked number one in his or her division. The average event revenue for these events was [REDACTED] but revenues ranged widely from [REDACTED] [REDACTED].⁷⁰ Given this, it is either true that not all top-ranked athletes are equally valuable in terms of their revenue-generating abilities (in which case weighting them equally is inappropriate), or there are other factors aside from the ranking of the top-ranked athlete in an event that contribute to event revenue (in which case, Dr. Singer's regression is uninformative about the appropriateness of his inverse rank weights), or both. Thus, Dr. Singer's event revenue regression provides no support for his inverse rank weighting scheme.

46. The second new analysis that Dr. Singer includes in his rebuttal report is a regression of average athlete compensation on the inverse of the athletes' rankings.⁷¹ To estimate this regression, Dr. Singer first calculates the average compensation by athletes' ranking across all Zuffa events in the Class Period.⁷² He then regresses this measure of average compensation—

⁷⁰ SINGER REBUTTAL BACKUP.

⁷¹ SINGER REBUTTAL REPORT at ¶ 130.

⁷² Dr. Singer excludes the winner's purse, fight of the night, performance, and knock-out of the night bonuses from his measure of compensation for this analysis. Dr. Singer is correct to exclude performance-based compensation from this analysis, since rank and performance pay may be jointly determined.


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notably in dollar terms rather than in terms of compensation as a share of event revenue—on the inverse of the athletes’ ranking, and finds a positive and statistically significant correlation.

47. At most, Dr. Singer’s new regression documents that, on average, higher-ranked athletes are paid more than lower-ranked athletes—no surprise there. Yet again, this obvious fact is not what Dr. Singer needs to demonstrate to justify his use of the inverse of athletes’ ranking in calculating his “foreclosure share.” Dr. Singer’s analysis also misleadingly masks the variation in compensation at the individual athlete level by running the regression using average compensation by ranking. When these data are disaggregated, it is clear that rank is not a monotonic predictor of compensation. In bout-level data, using a measure of compensation that excludes performance-related pay,⁷³ for 32 percent of bouts in the Class Period, the athlete with the higher rank earned *less* than the lower-ranked athlete, and in 17 percent of bouts, the higher ranked athlete earned the same amount as the lower-ranked athlete. A similar pattern is present in bouts featuring two Headliners: the higher-ranked athlete earned *less* than the lower-ranked athlete in 40 percent of bouts featuring two Headliners. Thus, for close to half of all bouts in the Class Period, Dr. Singer’s rank-based weighting assigns a higher value to the athlete who was paid less, or the same amount, by Zuffa.

VI. CONCLUSION

48. This report contains a statement of my opinions regarding the new expert opinions offered by Dr. Singer in his rebuttal report. The report also describes the bases for my opinions, the data and other information that I considered in forming them, and the analyses that I prepared to support these opinions.



Robert H. Topel, Ph.D.
February 12, 2018

⁷³ I use the same measure of compensation as Dr. Singer, however I also exclude the discretionary bonus which is awarded based on the quality of the fight.

APPENDIX A: CURRICULUM VITAE

Robert H. Topel

CURRICULUM VITAE

February 2018

CURRENT POSITIONS

Isidore Brown and Gladys J. Brown Distinguished Service Professor of Economics,
Booth School of Business, The University of Chicago
Director, George J. Stigler Center for the Study of the Economy and the State
Co-Director, Energy Policy Institute at Chicago (EPIC)
Research Associate, National Bureau of Economic Research

EDUCATION

B.A. (with High Honors), University of California, Santa Barbara, 1974
Ph.D., University of California, Los Angeles, 1980

FIELDS OF SPECIALIZATION

Microeconomics, Labor Economics, Industrial Organization, Health Economics,
Economic Policy, Energy Economics, National Security Economics

PREVIOUS ACADEMIC POSITIONS

Professor of Economics, Graduate School of Business, University of Chicago, 1986-93
Kirby Distinguished Visiting Professor of Economics, Texas A&M University, 2006
Professor of Economics, Department of Economics, University of California, Los Angeles, 1986
Associate Professor of Economics, Department of Economics, University of California, Los Angeles, 1985-86
Associate Professor of Economics, Graduate School of Business, University of Chicago, 1983-85
Assistant Professor of Economics, Department of Economics, University of Chicago, 1980-83

OTHER AFFILIATIONS

Research Associate, National Bureau of Economic Research, 1984-present
Senior Fellow, the Milken Institute, 1999-present
Faculty Member, MacLean Center for Clinical Medical Ethics, The University of Chicago
Member, National Petroleum Council Taskforce on Transportation Fuels Supply and Infrastructure, 2010-2012
Fellow, Center for the Study of Poverty and Inequality, Stanford University, 2006-present

Member, Brookings Panel on Economic Activity, various years
Visiting Scholar, Board of Governors of the Federal Reserve, 1990
Research Associate, Economics Research Center, NORC, 1980—1990
Consulting Economist, The Rand Corporation, 1982—1989
Research Associate, Center for the Study of the Economy and the State, 1980—present
Faculty Research Fellow, National Bureau of Economic Research, 1981-83
Research Economist, Unicon Corporation, 1981-88
Consultant, U.S. Department of Labor, 1985-90
Partner, Chicago Partners LLC 1994-2008
Principal & Managing Director, Navigant Economics, 2008-2013
Board of Directors, Ingalls Hospitals and Ingalls Health Service, 2000-2012
Director, WGA Evans Scholars Foundation, 2011-present
Senior Consultant, Charles River Associates, 2013-present

EDITORIAL POSITIONS

Editor, *Journal of Political Economy*, 1993-2003
Board of Editors, *American Economic Review*, 1992-94
Associate Editor, *Journal of Labor Economics*, 1982-92
Editorial Board, *International Journal of the Economics of Business*, 1993-present
Member of the Advisory Board, ERN Labor Journals, 1998-present

HONORS & AWARDS

Kenneth J. Arrow Award, International Health Economics Association, 2007
Kirby Distinguished Visiting Professor, Texas A&M University, 2006
Research America Eugene Garfield Prize for Medical and Health Research, 2005
Elected Fellow, Society of Labor Economists, 2004
Elected Member, Conference on Research in Income and Wealth
Elected Founding Member, National Academy of Social Insurance
William Ladany Research Scholar, The University of Chicago, 1989-91
William Fishman Research Scholar, The University of Chicago, 1986-87
Smith Richardson Dissertation Fellowship in Political Economy, 1978-79
Foundation for Research in Economics and Education Fellowships, 1975-79
Chancellor's Intern Fellow, University of California, Los Angeles, 1975-79
University Fellow, Northwestern University, 1975
General Electric Dissertation Fellowship, 1978

TEACHING EXPERIENCE

Graduate Economic Theory I, II, III
Law, Economics and Business
Competitive Strategy
Advanced Topics in Labor Economics
Advanced Topics in Microeconomics
Managing the Workplace
Industrial Organization/Antitrust
Price Theory

OTHER PROFESSIONAL ACTIVITIES

Thompson Lecture (Keynote Address), Midwest Economic Association, 2000
Nominating Committee, American Economic Association, 1996, 1997
Program Committee, American Economic Association, 2006-2007
Organizer, Universities-NBER Research Conference: "Labor Markets in the 1990s," Cambridge, December 1989
Program Chair, Labor Economics, Econometric Society Meetings, December 1989
National Science Foundation Review Panel in Economics, 1998, 1999
Inaugural Keynote Lecture, Missouri Economics Conference, University of Missouri, 2001
Pihl Lecturer, Wayne State University, November, 2004
Keynote Address, Federal Reserve Bank of Cleveland Conference on Education and Economic Development, November, 2004
Kirby Lecturer, Texas A&M University, 2006
Huggins Lecturer, Department of Surgery Huggins Conference, The University of Chicago, May, 2007
Keynote Address, Conference Board of Canada Conference on Medical Research, Montreal, January 2009
Keynote Address, Council on Competitiveness Conference on Energy Policy, Argonne National Laboratory, May 2009
Keynote Address, The University of Chicago/RFF/University of Illinois Conference on *Energy Policy and the Economy*, Washington, D.C., January 2010
Keynote Address, Humana Health Economics Forum, Santa Fe Institute, 2011
Keynote Address, Toyota Sustainability Conference, La Jolla, 2011
Keynote Address, Conference on Health Policy, Arizona State University, 2013

UNIVERSITY SERVICE

Director, Undergraduate Program in Economics, 1980-83
Chairman, Graduate School of Business Curriculum Review, 1990-91
Committee on Graduate Education, 1992-94
Committee on Undergraduate Education, 1993-94
Council of the University Senate, 1992-94, 1995-97, 1999-2002, 2004-07, 2010-13
Committee of the Council of the University Senate, 2000-02, 2006-07
Chairman, Council of the University Senate Committee to Review and Interpret the University Statutes, 2012-13
Graduate School of Business Policy Committee, 1995-97, 1999-2001
Member, Presidential Search Committee, 1999-2000
Board of Directors, The University of Chicago Laboratory Schools, 1986-92, 1998-2007
Chairman, Director Search Committee, The University of Chicago Laboratory Schools, 2002-2003
Area Coordinator, PhD Program in Economics, 2002-2008
Director, George J. Stigler Center, 2007-2015
Director, The University of Chicago Energy Initiative, 2008-2010
Co-Director, Energy Policy Institute at Chicago, 2010-present

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Labor Market Data and Measurement, with John Haltiwanger and Marilyn Manser. Chicago: The University of Chicago Press for NBER, 1998.

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APPENDIX B: MATERIALS RELIED UPON**Academic Articles and Other Studies**

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